*Further Remarks on Mr. Stone's Alterations of Bessel's Refractions.*
By W. H. M. Christie, Esq.

Mr. Stone, in his Reply to my paper " On Mr. Stone's Altera-
tions of Bessel's Refractions," published in the March No. of
the *Monthly Notices*, vol. xli. pp. 272–281, has tacitly conceded so
many of the points which I have urged that I should hardly
have considered it necessary to say anything further on the sub-
ject if it had not been that he asks for explanations of some of
my statements.

With reference to paragraphs 1 and 2 in the remarks on p. 95
of my paper, I can only express my regret that they have been
misunderstood.   They were not intended to cast any reflection on
Mr. Stone, but to call pointed attention to the omission of a
number of circumpolar observations from the collected results in
the Latitude Investigation given in the Greenwich " Introduc-
tion."   With the exception of the years 1857, 1858, and 1859,
the invariable practice has been, I believe, to collect all the results
of circumpolar observations in the Latitude Investigation.   As
I am not the only person who has felt a difficulty from the ab-
sence of all explanation in Mr. Stone's original paper on Bessel's
Mean Refractions of the source from which he derived his results,
and as others might be misled by the omission of the observa-
tions from the Greenwich " Introductions," I left the statement of
my difficulties as it originally occurred to me, and gave the
explanation afterwards.   I should be sorry to think that my
statement would bear such a construction as Mr. Stone seems
to put on it.   It was meant to be explanatory, not critical, for
it is obvious that serious criticism would be wasted when Mr.
Stone himself is reduced to saying " I have felt throughout that
I must have applied all the corrections which could be applied
with certainty to the results."   Even now Mr. Stone does not
say whether he used the old law for R—D, or the new, or a mix-
ture of the two.   Mr. Stone had shown to his own satisfaction
that his results were right and mine were wrong, and under
these circumstances I desired to explain that there was nothing
incompatible in two sets of results obtained with different data
and different corrections.   I do not attach the same importance
to these particular observations that Mr. Stone does, and I re-
gard them as simply a small portion of the chain of evidence on
the R—D correction and Bessel's Refractions.

The next point that calls for explanation has reference to
Mr. Stone's remarks on pp. 275, 276, in which, after imput-
ing to me a statement which I have not made, and which, in
fact, conveys a totally different meaning from my actual words,
he proceeds to call attention to a serious fallacy which invalidates
my work upon these questions of refractions.   The fallacy in
question is to be found in the words which Mr. Stone puts into
my mouth, but not in what I have actually said.   Mr. Stone

argues that *Bessel's Refractions unaltered* do not represent the Greenwich observations, because, as Mr. Main has shown, altera-tions are required beyond Z.D. 82°. This is perfectly true, and, as Mr. Stone well knows, I have over and over again stated that I adopt Mr. Main's corrections at very low altitudes, considering that they show Bessel's *law* of refraction to be erroneous. This, however, does not in the least affect the question of the accuracy of Bessel's *constant* of refraction, or, as it is sometimes expressed, Bessel's *mean* refractions. Mr. Stone's argument, therefore, falls to the ground unless he can show that I have maintained that *Bessel's Refractions unaltered* represent the Greenwich ob-servations. He quotes me thus, p. 275 (the italics are mine):—

"Mr. Christie considers my statement 'that the accumulated evidence of the Greenwich circumpolar observations is against the accuracy of *Bessel's Refractions unaltered*,' as 'really quite misleading.'"

What I really said was this, under the heading "First, as regards the value of the *constant* of refraction," on p. 97:— "In the first place, he [Mr. Stone] brings forward his dis-cussion of the observations 1857–1865, and appeals to his residuals and to the accumulated evidence of the Greenwich circumpolar observations in favour of his diminution of the *mean refractions*;" and further on in the same paragraph I add:—"So that it is really quite misleading to speak of the accumulated evidence of the Greenwich observations." The reference here is to Mr. Stone's statement in his paper "On *Mean* Refractions," April 1880, *Monthly Notices*, vol. xl. p. 336 :—"The recent Cape Observations do appear to offer grounds for serious doubts about the necessity of decreasing Bessel's Refractions, *for small zenith distances*, by 0·0053; but the evidence thus collected is not cer-tainly such as should outweigh the accumulated evidence of the Greenwich circumpolar observations."

There is another statement made by Mr. Stone which does not seem to be characterised by the accuracy which is desirable in such matters. On p. 276 he makes this statement: "With respect to the periods 1851–1861, 1862–1865, Mr. Christie has unfortunately grouped together observations reduced with Bessel's Refractions and observations reduced with refractions obtained by throwing away from Bessel's Refractions quantities varying from 0″·9 to over 2″." This statement is incorrect as regards the facts, and is calculated to convey the erroneous im-pression that I had not applied corrections to reduce the obser-vations to the same set of refractions throughout. In the first place, the actual corrections range from 0″·9 to 1″·12, *not* "from 0″·9 to over 2″," and the appropriate correction has been applied to each individual result, so that there is no error from this cause ; and, in the second place, the observations 1862–1865 have *all* been reduced with the same refractions—viz. Bessel's with Main's corrections below Z.D. 82°. It is curious that Mr. Stone should himself have done the very thing for which he reproaches

me, for he has grouped together observations above 82° Z.D. and observations below 82° Z.D. in his last group N.P.D. 43° to 46° 29′, and, what is a more serious oversight, he has grouped together observations reduced with two different laws for R−D discordance, without, as far as I can make out, applying any correction for the difference, which amounts to 0″·36. With reference to my selection of the groups of years 1851–1861 and 1862–1865, I may explain that one of the principal objects of my paper on the Systematic Errors of the Greenwich N.P.D.'s was to examine the validity of the R−D correction, and that the law was changed at the beginning of 1862. I did not propose to myself any discussion of refractions at low altitudes, and the examination of the results which Mr. Stone obtained in 1867 from the observations 1857–1865 was quite an incidental matter. Had I intended to discuss the refractions below 80° Z.D., I should have adopted Mr. Main's exhaustive treatment rather than the method followed by Mr. Stone, and I should have included in the discussion the Greenwich circumpolar observations below Z.D. 85°, of which in the period 1857–1865, there were 33 above the Pole and 21 below. It would be interesting to know why Mr. Stone has omitted these observations, which have an important bearing on the special question which he discusses.

But though I do not profess to discuss the refractions at very large Z.D., for which the data accumulated since Mr. Main's Memoir seem hardly sufficient, I must take exception to Mr. Stone's assumption that observations extending to Z.D. 78½° are not sufficient for discriminating between Mr. Stone's *constant* of refraction and Bessel's. As a matter of fact, Mr. Stone's alteration would affect the residual at Z.D. 78½° by 1″·0. But my reason for not discussing the observations below Z.D. 78½° in the period 1840–1847 (I have gone as far as Z.D. 84° in the periods 1851–1861 and 1862–1865) is that I did not see that I could add anything to Mr. Main's exhaustive discussion of all the observations below Z.D. 70° in the period 1836–1854. As Mr. Stone studiously ignores my reference to this important investigation as supplementing my own work, I will here give the residuals obtained by grouping Mr. Main's results for individual stars, in connection with my residuals for the periods 1840–1847 and 1851–1861:

<div align="center">

*N.P.D. above Pole − N.P.D. below Pole.*

*With Bessel's Refractions and Main's Corrections below Z.D. 82°*

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| Mean N.P.D. | 2° | 7½° | 12° | 21° | 28° | 31° | 33° | 37° | 41° | 44½° |
|---|---|---|---|---|---|---|---|---|---|---|
| $\alpha$ | +″·15 | +″·05 | −″·07 | −″·18 | −″·07 | +″·02 | +″·37 | +″·20 | ″ | ″ |
| $\beta$ | | | | | | | +·13 ±·13 | +·22 ±·13 | −·22 ±·16 | −·09 ±·16 |
| $\gamma$ | −·10 | +·02 | +·04 | −·27 | | +·15 | | +·04 | +·02 | −·02 |

The residuals $\alpha$ and $\gamma$ are for the periods 1840–1847 and

1851–1861 ; the residuals $\beta$ are formed from Mr. Main's results, *Memoirs R.A.S.* vol. xxvi., Table viii. pp. 122–23, giving weights by the formula $W = \dfrac{1}{2\epsilon^2 + \dfrac{e^2}{n}}$, where $e$ is the probable casual error of an observation S.P., and $\epsilon$ (which is taken $= 0''\cdot 22$) is the probable error of the result above pole and the probable syste-matic error of an observation S.P. The probable error of the residual inferred from the weight of the group is appended with the sign $\pm$. Each of the groups here treated corresponds to two of Mr. Main's groups in his Table x. (cont.), p. 129.

I submit that these residuals afford stronger evidence in favour of Bessel's constant of refraction than those from the observations 1857–1865 do in favour of Mr. Stone's diminished constant. And I cannot help thinking that he should have weighed the evidence of these 19 years, from 1836–1854, a little more carefully before he proposed a change which has turned out so unfortunately. It is to be noted, too, that his residuals B are seriously affected by the assumed law for the R – D correc-tion, which I have given reasons for considering to be erroneous.

As regards his residuals C, 1868–1876, I must ask why he omits to give the weights of the several groups. I should ima-gine that no practical astronomer would accept Mr. Stone's residuals without inquiring what are the weights of the respec-tive groups, what corrections have been applied, and what residuals would be given by an alternative hypothesis. The im-portance of this last test I have illustrated by reference to Mr. Stone's discussion of the Melbourne circumpolar observations* as well as of those now in question. In both cases I have shown that Bessel's Refractions with Main's corrections below Z.D.82° give smaller residuals.

There is only one point which calls for further remark, and that is with reference to the adopted co-latitude, for the years 1868–1876. Here Mr. Stone has apparently misapprehended my remarks and has so paraphrased my words as to convey a very different meaning. What I said was, not that the apparent change in the co-latitude " is due *solely* to a better distribution of the weights," but that it "is simply due to a change in the system of weights *and* to the increased number of observations of wide circumpolars," and I add that it began " in 1872, when special attention was given to the observation of *wide* circum-polars." No doubt it seems very specious to use the co-latitude given by the observations, but if this effective co-latitude does not agree with the real co-latitude of the Greenwich Observa-tory, it seems to me to show that there is a systematic error in the observations, which we ought rather to try to detect than to

* There is a small printer's omission in my paper, *Monthly Notices,* vol. xli. p. 98, line 31 : " Each has a probable error corresponding to weight,' should read, " Each has a probable error corresponding to weight 1, the figure 1 having dropped out in printing.

mask by the method which Mr. Stone adopts. And I would point out that Mr. Stone does not use what he terms the effective co-latitude—*i.e.* the difference between the circle-readings for the zenith point and the polar point—for that can only be given by observations of stars close to the polar point and not by stars 20° from this point. As I have pointed out, the close circumpolar stars show no change in the co-latitude, and thus there is no change in the difference of circle-readings for the zenith point and the polar point of the circle. But there is a change of nearly 1″ (as the curve given in my paper " On the Systematic Errors of the Greenwich N.P.D.'s " shows) in the difference of circle-readings for the zenith point and a point 22° below the pole at Z.D. 68° north. And this has nothing to do with the adopted zenith points, since the same change is shown in the distance between the polar point and the point 22° below the pole. Mr. Stone's argument that if the method of correcting the nadir point was perfectly satisfactory, then the co-latitudes determined from the observations should be perfectly satisfactory, amounts to this, that if one thing is right other things cannot be wrong. I am glad, however, to find that Mr. Stone does not now dispute the validity of the method of correcting the nadir-point observation.

I now come to the statement which I gave respecting the errors of the micrometer screws, and which Mr. Stone thinks is not made with the precision which is desirable.

The statement in question—" In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at $0^r$ of the microscope-micrometers "—means that the following notice was put up in the Transit Circle room: " The Nadir Point observation is to be taken at 179° 40′ + $0^r$." As Mr. Stone is, I believe, responsible for this notice, no doubt he can give its precise meaning. In my paper " On the Effect of Wear on the Micrometer Screws of the Greenwich Transit Circle " (*Monthly Notices*, vol. xxxvii. p. 20) it was sufficient for me to show that there was a preponderance of a large number of observations between $0^r$ and $1^r$ of Microscope A as it read in 1875, or between $0^r\cdot2$ and $1^r\cdot2$ of Microscope A as it read during a considerable part of 1868. An examination of the curves I have given will show that when the corrections indicated in the text (p. 20) are applied to the 1868 curve (viz. $-0''\cdot4$ approximately at $0^r$, and $+0''\cdot2$ at $5^r$, with intermediate corrections between these two points) the 1875 curve begins to rise above the 1868 curve (indicating increased wear of the screws) at about $0^r\cdot4$ of Microscope A as it read in 1868, and does not meet this curve again till about $1^r\cdot2$. It must be understood, however, that no great precision is to be expected in such a case, on account of accidental errors in the curve, and I have therefore contented myself with specifying the part between $0^r$ and $1^r$ as indicating extra wear since 1868. I have nowhere said anything about readings of Microscope A less than $0^r\cdot2$, and therefore Mr.

Stone's interesting collection of such readings does not in the least concern me.    Let us now take the nadir observations 1868, Feb. 3, to 1869, Dec. 31, with reference to my actual statement, and not with reference to Mr. Stone's interpretation of it. In the period cited there were 559 determinations in all : 543 of these were made at readings (of Microscope A) $1^r\cdot2$ and under, 520 at readings $1^r\cdot0$ and under, 488 at readings $0^r\cdot9$ and under, 448 at readings $0^r\cdot8$ and under, 334 at readings $0^r\cdot6$ and under, and 283 at readings $0^r\cdot4$ and under.    I think it is evident from this that there was a large preponderance of observations in the part of the screws I have specified, more especially since the nadir-point observation was usually taken twice a day in the latter part of the period 1868–1875.    It seems a pity that Mr. Stone did not make himself better acquainted with the facts before making the confident assertion that " whatever may have been the state of the screws near $0^r\cdot0$ in 1876, it certainly was not caused by the 4,000 observations made near $0^r\cdot0$ at my suggestion."

With regard to the practice introduced in 1868 of taking the nadir point at $0^r$, Mr. Stone states that it was done to avoid very large corrections for run.    I had heard this reason given, but I should have been loath to attribute it to Mr. Stone, for it implies the serious fallacy that the real reference point for runs is $0^r$ instead of being about $2^r\cdot5$, the middle of the range over which the observations are distributed.    It is perfectly true that for mere convenience of computation the correction for runs is reckoned from $0^r$ so as to avoid negative signs, but the real correction for runs, as affecting any particular observation, is the difference between the correction reckoned from $0^r$ and the average correction for observations of stars.    I had stated this in the very passage which Mr. Stone quotes, and had given it as the reason for my suggesting that the nadir observation should be taken about $2^r\cdot5$, but as Mr. Stone takes no notice of it, I am obliged again to call attention to the point.

After this statement of Mr. Stone's I am not so surprised as I should otherwise have been at the difficulty he experiences in understanding how the corrections for runs enter into the corrections for errors of micrometer screws, though I thought I had sufficiently explained the matter in my paper of 1876.    Mr. Stone is quite right in supposing that the runs had not been corrected for errors of screws before their application, but he is quite wrong in supposing that there was any oversight in the matter, and by his implying that the runs ought to have been so corrected for errors of screws he seems to me to show that he does not appreciate the nature of the problem.

The problem really was this : To determine the corrections applicable to the Greenwich N.P.D.'s on account of errors of the micrometer screws, these N.P.D.'s being affected by error of the screws at the point at which the observation was made, and by the error in the runs resulting from error of the screws at $0^r$ and